in the moving matter. This means a moving force $-(U_1/c_1)\nabla c_1$. But if there is compression, c_1 probably always varies intrinsically as well.

It will be found that the omission of the auxiliary $\mathbf{h}$ has the result of complicating instead of simplifying the force formulæ. Similarly the omission of $\mathbf{e}$ complicates them. Now the use of $\mathbf{e}$ is founded upon the idea that the electric polarisation is produced by a separation of ions under the action of $\mathbf{E}_1$ for $\mathbf{E}_1$ is the moving force on a moving unit electric charge. Analogously $\mathbf{h}_1$ is the moving force on a moving unit magnetic charge or magneton. If there are really no such things, the interpretation must be made equivalent in other terms. But the categorical imperative is not easily to be overcome.

The application to plane waves I described in a recent letter (NATURE, March 9) will be found to harmonise with the above in the special case.

But a correction is needed. In the estimation of the moving force on "glass" receiving radiation, the assumption was made that the electric and magnetic energies in the transmitted wave were equal. So the result is strictly limited by that condition. The conditions $E=wB$ and $U=T$ are not coextensive in general, though satisfied together in Lorentz's case. When U not $=T$, we have instead of (8), p. 439,

$$p_1v-p_2v-p_3w=w(T_3-U_3),$$

and the rate of loss of electromagnetic energy is

$$2\mu H_1H_2u+(w-u)(T_3-U_3).$$

Now this is zero when $e=0$, or the polarisation is proportional to the electric force. The question is raised how to discriminate, according to the data stated above, between cases of loss of energy and no loss. To answer this question, let $\mathbf{e}$ and $\mathbf{h}$ in the above be unstated in form; else the same. Then, instead of (4), the activity equation will be

$$-\nabla\mathbf{W}=\dot{U}+\dot{T}+\{\tfrac{1}{2}E^2(\partial c_0/\partial t)+\ldots\}+(\mathbf{f}_0\mathbf{q}+\mathbf{f}_1\mathbf{u})-(\mathbf{e}\mathbf{J}_1+\mathbf{h}\mathbf{G}_1), \quad (5)$$

where $\mathbf{W}$ is as in (4), whilst $\mathbf{f}_0$ and $\mathbf{f}_1$ are the forces derived from the stresses specified (not the same as $\mathbf{F}_0$ and $\mathbf{F}_1$), and $\mathbf{J}_1$, $\mathbf{G}_1$ are the electric and magnetic polarisation currents, thus, $\mathbf{J}_1=\mathbf{D}_1+V\nabla\mathbf{h}_1$, &c. It follows that it is upon $\mathbf{e}$ and $\mathbf{h}$ that the loss of energy depends in plane waves, when $\mathbf{u}$ and $\mathbf{q}$ are constant. For the stresses reduce to longitudinal pressures, so that by line integration along a tube of energy flux we get

$$\Sigma(eJ_1+hG_1)=\Sigma(U+T). \quad (6)$$

Thus, when a pulse enters moving glass from stationary ether, the rate of loss of energy is $\Sigma(-eJ_1)$. If e is zero, so is the loss, as in the special case above. There is also agreement with the calculated loss in the other case. That the moving force on the glass should be controlled by $\mathbf{e}$ is remarkable, for it is merely the small difference between the electric force on a fixed and a moving unit charge. The theory is not final, of course. If the electromagnetics of the ether and matter could be made very simple, it would be a fine thing; but it does not seem probable.

OLIVER HEAVISIDE.

April 5.

The Dynamical Theory of Gases.

IN a letter to NATURE (April 13) Lord Rayleigh makes a criticism on my suggested explanation of the well known difficulty connected with the specific heats of a gas. He considers a gas bounded by a perfectly reflecting enclosure, and says "the only effect of the appeal to the æther is to bring in an infinitude of new modes of vibration, each of which, according to the law (of equipartition), should have its full share of the total energy."

The apparent difficulty was before my mind when writing my book. Indeed, as Lord Rayleigh remarks, something of the kind had already been indicated by Maxwell. (I think the passage to which Lord Rayleigh refers will be found in the "Coll. Works," ii., p. 433:—"Boltzmann has suggested that we are to look for the explanation in the mutual action between the molecules and the æthereal medium which surrounds them. I am afraid, however, that if we call in the help of this medium, we shall only increase the calculated specific heat, which is already too great.") It seemed to me, however, that the difficulty was fully met by the numerical results arrived at in chapter ix. of my book.

Suppose, to make the point at issue as definite as possible, we take a sample of air from the atmosphere, say at 15° C. Almost all the energy of this gas will be assignable to five degrees of freedom—so far as we know, three of translation and two of rotation. Let us surround this gas by an imaginary perfectly reflecting boundary. The total energy of matter and æther inside this enclosure will remain unaltered through all time, but this total energy may be divided conveniently into two parts:—

(1) The energy of the five degrees of freedom, say A.

(2) The energy of the remaining degrees of freedom of the matter plus the energy of the æther, say B.

As Lord Rayleigh insists, the system is now a conservative system, so that according to the law of equipartition, the total energy A+B is, *in the final state of the gas,* divided in the ratio

$$A:B=5:\infty \quad . \quad . \quad . \quad . \quad . \quad (1)$$

whereas observation seems to suggest that the ratio ought to retain its initial value

$$A:B=5:0 \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This I fully admit, but a further point, which I tried to bring out in the chapter already mentioned, is that the transition from the ratio (2) to the ratio (1) is very slow—if my calculations are accurate, millions of years would hardly suffice for any perceptible change—so that, although (1) may be the true final ratio, it is quite impossible to obtain experimental evidence of it.

If the sample of gas were initially at a much higher temperature than we have supposed, the transition would undoubtedly be much more rapid; but even here we could not hope for experimental verification. For the assumed boundary, impervious to all forms of energy and itself possessing none, cannot be realised in practice, and as soon as the energy of the enclosed æther becomes appreciable, the imperfections of our apparatus would become of paramount importance in determining the sequence of events.

J. H. JEANS.

Growth of a Wave-group when the Group-velocity is Negative.

THE following may be of interest in connection with the recent discussion on the flow of energy in such cases.

Let the energy of an element of a linearly arranged mechanical system be

$$\{(d^2y/dxdt)^2+y^2\}dx/2.$$

Such a system can be approximately realised by taking a bicycle chain, loading it so that the radius of gyration of each link has the same large value, and suspending it by equal threads attached to each link so that the chain is horizontal and the axes of the links vertical. By the principle of least action we immediately find the equation of motion to be $d^4y/dx^2dt^2=y$. A simple harmonic wave is given by $y=\sin(pt-x/p)$. The group velocity is $-p^2$, and is negative. Let such a system, extending from $x=0$ to $x=\infty$, be at rest in its position of equilibrium at time $t=0$, and then let the point $x=0$ be moved so that its position at any subsequent time is given by $y=1-\cos t$.

By application of the usual method *viâ* Fourier's integral, the motion of the system is found to be given by either of the equivalent formulæ

$$y=\Sigma(-1)^n(t/x)^{n+1}J_{2n+2}2\sqrt{(tx)},$$

or

$$y=1-\cos(t+x)-1+\Sigma(-1)^n(x/t)^nJ_{2n}2\sqrt{(tx)},$$

where the J's are Bessel's functions and the summations extend from $n=0$ to $n=\infty$. There are some doubtful

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points in the reasoning, however, and the proof consists in showing (1) that y satisfies the differential equation, (2) from the second formula that $y=1-\cos t$ when $x=0$, (3) from the first formula that y and dy/dt are both zero when $t=0$, (4) from the first formula that when t is finite y is small for all large values of x. If, now, x is finite and t great, the second formula reduces to $y=-\cos(t+x)$, so that the motion now consists entirely of waves proceeding *towards* the source of the disturbance—a most remarkable result. If in the formulæ for y we change the sign of x, the J functions are replaced by I functions. The resulting value of y does not satisfy (4), and cannot be accepted as a solution of the problem.

H. C. POCKLINGTON.

The Transposition of Zoological Names.

AMONG the many radical changes in zoological nomenclature proposed of late years, none appear to me more open to objection than those where names which have long been in general use for particular species or groups are transferred to others on the ground that they were originally applied to the latter. One of the earliest of such transpositions was suggested by Prof. Newton, of Cambridge, who urged that Strix is not the proper generic designation of the barn-owl, and that while this species should be called *Aluco flammeus,* the tawny owl should take the generic title Strix, as *S. aluco.* I find, however, that this emendation is not accepted in the British Museum "Hand-list of Birds," where the barn-owl figures under its familiar title of *Strix flammea.* Uniformity is not, therefore, attained by this proposal.

Another instance occurs in the case of the walrus, which was long known as *Trichechus rosmarus,* until systematists discovered that the generic title refers properly to the manati, to which animal they transferred the name. Again, the *Simia satyrus* of Linnæus is now stated to be the chimpanzi, and not the orang-utan, and consequently Simia is made to stand for the latter instead of for the former. As a fourth example of this transference of a familiar generic name may be cited the case of the marmosets of the genus Hapale, to which it is now proposed to apply the title Chrysothrix, despite its practically immemorial use as the designation of the titi monkeys.

As an example of the transference of a species name, it will suffice to take the case of the African antelope commonly known as the white oryx (*Oryx leucoryx*). This name, it is stated, properly belongs to the Arabian Beatrix oryx, to which it is accordingly proposed that it should be transferred, after being so long used for the former animal.

Personally, I am very strongly of opinion that such transpositions should not on any account be permitted, and that when a species or genus has been known by a particular name for a period of, say, fifty years, this should, *ipso facto,* give such an indefeasible title to that name (altogether irrespective of its original application) as to bar its transference to any other group or species. It may, indeed, be deemed advisable that, as in the case of the walrus, the old name should not be retained in the generally accepted sense, but, if so, it should be altogether discarded, and not transferred. The practice of transferring names must, if persisted in, inevitably lead to much unnecessary confusion without the slightest compensating advantage. Indeed, it will render such works as Darwin's "Origin of Species" and Wallace's "Geographical Distribution of Animals," which are certain to live as biological classics, absolutely misleading to the next generation unless special explanatory glossaries are supplied.

Advanced systematists urge that those who refuse to follow their lead in this and other kindred emendations in nomenclature are not only old-fashioned and behind the times, but that they are absolutely doing their best to hinder the progress of zoological science. This, however, is but the opinion of a comparatively small (and, shall we say, somewhat prejudiced?) section. What we really want is the opinion of all those interested in zoology and natural history, namely, professional zoologists, palæontologists, geologists, physiologists, anatomists, zoogeographers, amateur naturalists, and sportsmen. If the general consensus of opinion of all these were on the side of the proposed changes, and of others of a similar type, then, and then only, I venture to think, could they be regarded as obligatory.

It may be added that the use of combinations, which Mr. Stebbing has felicitously designated "comicalities in nomenclature," of the type of *Anser anser* and *asinus asinus* (or, still worse, *Asinus asinus asinus,* which is a possible contingency), is rapidly tending to discredit the common sense of scientific zoologists among matter-of-fact men of the world.

R. LYDEKKER.

A little known Property of the Gyroscope.

To my surprise I have found that the property of the gyroscope which I am about to describe, although perfectly elementary, appears to be little known to either physicists or astronomers. Neither is it mentioned in the text-books so far as I am aware. That it has a very important bearing on the mechanism of the solar system has been shown in some of my earlier papers, but the laws which govern the rotation and the simple facts themselves seem to be so little understood that I have thought it worth while to explain them more fully in this place.

If a gyroscope is mounted on gimbals so that it may shift its plane of rotation freely about an axis passing through the plane of the revolving disc, we shall find it is possessed of certain curious properties. To most persons the notable characteristic of a gyroscope is the resistance it offers to any force tending to change the plane of its rotation. This is true of it only, however, in case certain conditions are complied with. If these conditions are neglected, it will change its plane with the greatest facility.

If the wheel is properly balanced and mounted as above described, and we set it spinning, it will continue to rotate in one plane without change until it stops. Suppose that while it is spinning we set it upon a table, and cause the stand supporting it to revolve slowly about its vertical axis. Instantly the wheel will adjust itself so as to revolve in a plane parallel to the surface of the table.

Furthermore, the direction of rotation of the wheel upon its axis will be the same as the direction of rotation of the stand. If we turn the stand in the opposite direction the wheel will at once shift its plane, and turn over, so as again to rotate in the same direction as the stand.

Another way of showing the experiment is to hold the stand supporting the gyroscope at arm's length. The observer then slowly revolves upon his heels, first in one direction and then in the other. Each time the observer shifts his own direction of motion the gyroscope will shift its plane, and always in such a manner that its direction of rotation shall be parallel and in the same direction as its revolution in its orbit.

It is a well known fact that according to the nebular hypothesis all the planets should have rotated in a direction opposite to that of their revolution in their orbits, just as Neptune does at the present time. This is because by Kepler's laws the inner edge of a revolving ring must necessarily move faster than the outer edge. The fact that Neptune is the only planet that even approximately fulfils this condition has always been a source of trouble to the adherents of the nebular hypothesis. No one has ever even attempted to explain the anomalous rotation of Uranus, in a plane practically perpendicular to the plane of its orbit.

The interesting property of rotating bodies illustrated above in the case of the gyroscope, and fully explained by its theory, now at once makes the matter perfectly clear. In the case of the planetary bodies, the force rotating the stand of the gyroscope is supplied by the annual tide raised upon the planets by the sun. In former times, when the planets were large diffuse bodies, this tidal force was of considerable importance. Neptune, however, is so remote from the sun that the tidal influence upon it has always been small. The plane of its rotation, therefore, has been but slightly shifted from that of its orbit—about 35°. Uranus being nearer the sun has had its plane shifted nearly half-way over, or through 82°. The plane of rotation of Saturn has been shifted through 153°, while that of Jupiter has suffered a nearly complete reversal, and the planet now revolves approximately in the plane of its

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